

Interactive comment on “Transition of flow regime along a marine-terminating outlet glacier in East Antarctica” by D. Callens et al.

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Dear Editor,

We would like to thank both referees for the thorough job on judging our paper submitted to TCD. Please find below our answers to the query of both referees of our paper. Especially Referee #1 had a number of remarks that made us change some sections of the manuscript in a more profound way. Therefore, the section on numerical modelling has undergone a complete overhaul, in which we used a simple SIA model (following the advice of Referee #1) and applied an inverse modelling technique to infer the

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basal conditions. This is a more sound approach than the one previously designed (more complex model, but results simply compared to observations). This also led us to take another co-author on board (Emmanuel Witrant), who is specialized in inverse problems and currently collaborating with us at ULB.

Sincerely,

Denis Callens

R.G. Bingham (Referee) r.bingham@ed.ac.uk Received and published: 23 November 2013

Specific comments

[REF] Q1. One of the themes of the paper is that the mass flux of WRG is more significant, and proportionally a greater component of mass outflow from DML, than previously known. This conclusion is apparently reached predominantly on the basis that the bed is deeper than previously interpolated, i.e. from BEDMAP (Lythe et al., 2001). Yet I feel there are some ambiguities to this conclusion. My first query is what actually was the estimated mass flux for WRG individually prior to this study? In Rignot et al. (2008) WRG is conflated with several other outlets along the Queen Maud Land coastline, so from that paper there is no individual estimate. I suspect from this there has been no published estimate for WRG individually; but nevertheless the authors could easily work out what it would be if one didn't have the new bed data, i.e. using the bed from BEDMAP to calculate mass flux from WRG. That way one would have a better sense of how much difference the new bed actually makes. (What I am getting at here is whether it's the new bed that makes us realise WRG is a bigger contributor than previously thought, or whether it's just been overlooked as a significant contributor because previous studies never actually looked at it individually.)

[AUTHOR] The referee is correct in saying that if we presume that WRG is a bigger contributor than previously known, we should demonstrate that it is. However, since

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there is no previously published material, it is nonsensical to repeat a calculation with data that are presumed to be wrong. We therefore rephrased a number of sentences. Instead of showing that WRG is probably bigger than presumed, we emphasize on the fact that the new study reveals that WRG is an important contributor and in view of its geometry may be considered as more prone to change compared to other main contributors in the DML sector.

[REF] My second query is how well do we know the bed for the flux gates of Shirase Glacier and Jutulstraumen? Is there a possibility that these beds too could be deeper than in BEDMAP's interpolation, and therefore that their mass flux is ALSO greater than previously thought – and that, therefore, as a proportion of mass flux from DML, that of WRG could still be less significant compared with Shirase Glacier and Jutulstraumen?

[AUTHOR] The referee is right that missing information may have an impact on the estimates we propose (essential stemming from the available literature). We are aware that AWI/JARE collected new data in the Shirase area (not specifically focussed on Shirase Glacier itself), but from the data availability of BEDMAP2, there seems no difference with BEDMAP. On the other hand, new data has been added for the Jutulstraumen area. Nevertheless, we made a back-of-the-envelope calculation of mass flux at the GL for those two glaciers, using Rignot's velocities and BEDMAP2 ice thickness data. If we consider plug flow, Jutulstraumen and Shirase Glacier drain 11 and 10 Gt/yr, respectively. This is very similar to the data found in the literature and cited in our manuscript. Furthermore, comparison with BEDMAP1 does not reveal any substantial difference. However, the ice thickness of Jutulstraumen was revised between BEDMAP1 and 2 and the glacier has been found substantially thinner than previously thought. Therefore, the back-of-the-envelope calculation gives a slightly lower mass flux than the one reported in the literature.

[REF] Thirdly, can the authors confirm that the comparison between Shirase/Jutulstraumen and WRG also uses satellite-derived ice-surface velocities across the flux gates from the same eras?

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[AUTHOR] The mass flux of Shirase is determined by interferometry. The estimation for Jutulstraumen is based on stake measurements. Both estimates date from a different time period. See also previous remark in which a quick calculation of mass flux based on two contemporary datasets virtually yields the same results.

[REF] Q2. It would be useful to have explained in the paper why the authors generated their own surface-velocity map from satellite data across the region, i.e. rather than using the MEASURES product of Rignot et al. (2011). It's a fine product but I just didn't appreciate from the paper as written what added value is given by doing this rather than using MEASURES. Was this a case of generating a product with improved spatial resolution for this area? Are the 2000 data actually the most recent for this area?

[AUTHOR] Rignot's product is a mosaic of velocity which is not always coherent in a specific region (some images in the same area or from quite different time periods). Using our own dataset ensures this coherence. Secondly, the resolution is more or less 2 times finer and we use speckle tracking as well as interferometry.

[REF] Q3. Much is made in the paper that the downstream (flatter/smooth) sector of WRG is "wide" and smooth and would leave the catchment open to ice-stream lateral migration. However, when I view both Figs. 2 and 3, what I see is that the eastern part of the catchment, all the way downstream, i.e. even in cross-profile 1, appears to be underlain by a fairly rough, undulating bed that is quite topographically high relative to further west. In other words I think the eastern part of the downstream section (< 65 km from the grounding line) actually appears more similar in bed character to the upstream section. There is still a smooth-ish, wide-ish downstream area from the centre to the west of the downstream profiles, but the way the manuscript is currently written, one would get the impression that it's wider than it is. It looks to me as if the rough bed to the east would limit the ice-stream's lateral migration, so from this dataset I'm less convinced than the authors imply in their discussion that this really is an analogy for the Siple Coast region.

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[AUTHOR] We may have been too far in linking WRG to the Siple Coast ice streams, but we do not agree completely with the line of reasoning of the referee. In any case, we should be more careful in laying out our case. Turning to the data, the vertical elevation change across the whole section of profile 1 situated near the grounding line is less than 100 m, over a distance of 120 km. In terms of East-Antarctic bed topography, this is smooth. The same smoothness is corroborated by the longitudinal profile, exhibiting a similar variability (even less) over a distance of 65 km. The profiles upstream from the grounding line show more variability and gradually one can distinguish the central trunk. However, the upstream section shows a topographic variability of 500-700 m over the same distance. This is clearly not smooth: the trunk is clearly identifiable in the data. We agree that profiles 4 and 5 are intermediate in nature, but for a glacier that speeds up towards the grounding line, hence increases its potential to erode with increasing distance from the ice divide, it is odd that towards the grounding line, the bedrock is actually not getting deeper, but becomes less pronounced even though the ice flow – as determined from interferometry – remains concentrated in width. There might be a geologic control on this (which we did not investigate, since it is not the scope of the paper). We are aware that a major (geologic) shear zone runs through the Sor Rondane Mountains in an east-west direction (Kojima and Shiraishi, 1986), but this does not coincide with a noticeable change in bedrock in our profiles (the shear zone is situated well in the upstream section of our longitudinal profile). We took care in diluting our statements linking WRG to Siple Coast ice streams.

[REF] Q4. Can the authors clarify why they did not present bed reflectivity data for the 7 transects across WRG to supplement its analysis along the single long profile. These data would help to test further my contention made in Q3.

[AUTHOR] The longitudinal profile includes reflections from the ocean surface at different flight altitudes, which gives absolute calibration of the system. This calibration is valid for the entire longitudinal profile. However, the cross-glacier profiles were collected with different system settings, so the system calibration mentioned above is no

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longer valid. They were also collected on a different day. Therefore, it is meaningless to compare measured bed returned power at cross-over points (anyway, they show a large discrepancy). Their relative value within a section may well be useful, but the absolute value is too different at the cross-over points with the longitudinal profile. Across the glacier, ice temperature is expected to vary significantly, though our modelling is along a flowline. In order to keep the high-resolution record, the flowline approach is the only one feasible, since a 3D approach would incur too much interpolation.

[REF] Q5. Can the authors also clarify why they present BRP(c) [the simple geometrically corrected BRP; Fig. 4b] AS WELL AS the englacial-attenuation-corrected BRP (Fig. 4d). As I understand this, one is simply making the first step towards the other, so I'm unclear as to why both are presented here, i.e. why not just present the geometrically AND englacial-attenuation corrected product (as in Fig 4d) alone? The only argument I could see for retaining the result in which englacial attenuation has not been included is that one may be insufficiently unsure of the assumptions that go into generating the attenuation estimate. However, as the results in Fig. 4b and d essentially match each other in all main respects, it seems that presenting Fig 4b and discussing it independently seems a little superfluous.

[AUTHOR] As the referee points out, adding the BRPc correction next to the englacial attenuation correction has the advantage to distinguish between the effects of both. Since it is just a panel amongst 4 panels represented, we don't think this is overkill, or that it takes up too much space in the paper. We therefore kept that figure in the paper. In this way, we are able to explicitly present the fraction of bed reflectivity and attenuation contributions to the measured bed returned power. Also, comparison between panels a (roughness) and b (returned power) clearly demonstrate that the bed roughness is not a primary control of the bed returned power.

Technical corrections

[REF] Firstly, this isn't a specific point, but I think the writing could be made more

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concise, and more precise, in several places. The introduction is quite long, and for a paper that essentially presents new data from East Antarctica, there is a lot of material on West Antarctica in the first paragraph. It is contextual, and I wouldn't drop it totally, but I would consider reordering the introduction a little.

[AUTHOR] We rewrote the first paragraph of the introduction to focus on the more specific aspect of the East Antarctic ice sheet. Furthermore, we added reference to the recent paper by Miles et al (2013).

[REF] As a further example, I do not think, with the general format this paper has followed (i.e. mini methods sections followed by mini-results/discussion sections) that both the final sections (7. Discussion and 8. Conclusions) are required. Section 8 just seem to provide unnecessary repetition. [AUTHOR] We corrected this.

[REF] Another issue to clean up is that there is much alternation between use of past and present tenses in various sections, and it would be better to try to stick to one type. [AUTHOR] Pg 4916 L12: Done [AUTHOR] Pg4916 L22 :Done [AUTHOR] Pg 4918 L7: Done [AUTHOR] Pg 4919 L1: Done [AUTHOR] Pg 4919 L2: Done [AUTHOR] Pg 4919 L3: Done [AUTHOR] Pg 4919 L5: Done [AUTHOR] Pg 4919 L22: Done [AUTHOR] Pg 4921 L20 : Done [AUTHOR] Pg 4922 L2 : Done

[REF] Throughout manuscript: check that all instances of "Antarctic Ice Sheet" are written with uppercase "I" and "S" [AUTHOR] Pg 4914 L23 : Done [AUTHOR] Pg 4915 L16 : Done

[REF] and all instances of West Ragnhild (or other) Glacier are written with capital "G". [AUTHOR] Done

[REF] Also I think it's unnecessary to use "the" in front of "West Ragnhild Glacier" which occurs in quite a few sections of the writing. [AUTHOR] Pg 4915 L25 : Done [AUTHOR] Pg 4916 L3: Done [AUTHOR] Pg 4916 L8: Done [AUTHOR] Pg 4916 L11: Done [AUTHOR] Pg 4916 L26: Done

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[REF] Throughout manuscript: check spelling is consistently UK English or US English – currently it's mostly UK English but some examples of US English (to be changed?) are color (UK = colour; especially this is in the figure captions text) and center (UK = centre). [AUTHOR] Pg 4917 L3 : center->centre [AUTHOR] Pg 4932 L6 : color ->colour [AUTHOR] Pg 4933 L2 : color ->colour [AUTHOR] Pg 4933 L4: center ->centre [AUTHOR] Pg 4934 L1 : color ->colour

[REF] Throughout manuscript: Some overuse of hyphens for East-Antarctic and West-Antarctic. [AUTHOR] We changed the running title [AUTHOR] Pg 4914 L22 : Removed [AUTHOR] Pg 4915 L16: removed [AUTHOR] Pg 4916 L19: removed

[REF] At some point, it might be noted that WRG is the same glacier/ice-stream as that which Rignot et al. (2011, p.1428) refer to as an "unnamed glacier: : :which for convenience is referred to as Sor Rondane: : :Ice Stream."

[AUTHOR] In the literature there has been quite some confusion about the glacier system in that area. Western Ragnhild Glacier has been used for the first time by Pattyn et al., 2005. Prior to that, Jezek et al (2001) named the glacier erroneously Byrdbreen, which is one of the outlet glaciers that flows through the Sor Rondane Mountains (not diverted around the range). The glacier has no official name, so more confusion is added by mentioning that it is either unnamed, or called the Sor Rondane ice stream (which is very ambiguous). Moreover, Rignot et al (2011) refer to the Pattyn et al (2005) paper, so they could have taken some more care in being consistent with the naming.

[REF] P4914 L11: Better written as: "...regions, and is consistent with the downstream bed being draped with sediment." [AUTHOR] Do not agree

[REF] P4914 L12: This is a little awkwardly written, and I think it would be easier in the abstract to remove the reference to 20 km and just say the BRP varies across the transition. Suggest: "varies by 30 dB across the bed-character transition, which: : :"
P4914 L13: Rewrite: " : : :that the water content at the bed/ice: : :"
[AUTHOR] done

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[REF] P4914 L14: “All the above lines : :” [AUTHOR] done

[REF] P4914 L17: “: :relatively: : :” [AUTHOR] done

[REF] P4915 L3: Also could reference Pritchard and Vaughan (2007) JGR here. [AUTHOR] This sentence has been removed all together

[REF] P4915 L8: I would replace “currently” with “conventionally” here, just to make it clearer that ideas are changing a little here now that it’s confirmed from several recent surveys that there are several substantial “marine” basins in East Antarctica too. [AUTHOR] Not relevant anymore, since this section has been profoundly reworked

[REF] P4915 L11: Very ambiguously written. It’s not “contact with the ocean” per se that makes the WAIS vulnerable. This needs a little rewrite. [AUTHOR] done

[REF] P4916 L26. No need for the brackets here. Just make this its own sentence; also rewrite as “: :250 km upstream from the grounding line. [AUTHOR] done

[REF] P4917 L1-16: Lots of use of present tense here where I would suggest using past tense – the survey is something that has been done in the past. [AUTHOR] corrected

[REF] P4917 L8: The Nixdorf and Steinhage references are over a decade old – can the authors clarify if the radar system and its way of operation really has remained unchanged over all this time, or should they be using a more recent reference? [AUTHOR] Indeed, the radar system is over a decade old; however, while adaptations have been done, the basic features of the system that are relevant for this paper, are the same.

[REF] P4917 L10: “: :data gaps slope down steeply: : :” “: :Therefore, the data: : :” [AUTHOR] Done

[REF] P4917 L12: “: :and hence loss of signal.” (instead of “which may explain: : :”) [AUTHOR] Done

[REF] P4918 L3-5: Slightly unsure as to what is meant by the landward valley between Sor Rondane and Belgica Highlands: : : Does this mean the downstream basin is

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flatter than the bottom of the deep valley along its long profile? [AUTHOR] Yes

[REF] P4919 L11-13: I’m unclear what this means. Smoother than what? [AUTHOR] more gradual than expected from visual interpretation

[REF] P4919 L24: “: :can be treated as a proxy for bed reflectivity: : :” [AUTHOR] Done

[REF] P4920 L1: “In the decibel scale: : :” [AUTHOR] Done

[REF] P4921 L11: singular, “field” [AUTHOR] removed

[REF] P4921 L22: Can you offer an explanation for why the attenuation decreases _ 20 dB just here? Further upstream you attribute the change to changing ice thickness, but proffer no explanation for this change here. [AUTHOR] Done

[REF] P4922 L25: I think “DEM” is a better word than “profile” here. [AUTHOR] This section has been rewritten (see major comments of Referee #1)

[REF] P4923 L1: “: :data have been: : :” [AUTHOR] done

[REF] P4923 L3-8: Clarify quantitatively what is short and what is long. [AUTHOR] done

[REF] P4923 L16: Replace “i.e.,” with “of” [AUTHOR] Section has been rewritten

[REF] P4923 L18: plural – “procedures” [AUTHOR] Section has been rewritten

[REF] P4923 L19: “: : (2012), which allow us to perform diagnostic runs: : :” [AUTHOR] Section has been rewritten

[REF] P4923 L24: “: :derived the corresponding: : :” [AUTHOR] Section has been rewritten

[REF] P4923 L26: Remove “However” (you use it again in the next sentence). Also remove “probably” in next line. [AUTHOR] Section has been rewritten

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[REF] P4923 L30: No hyphen in Fox Maule (and also change in reference list) [AUTHOR] done

[REF] P4924 L3: “: :heating have virtually: : :” [AUTHOR] done

[REF] P4924 L24: “: : :though the exact onset: : :” [AUTHOR] Section has been rewritten

[REF] P4925 L1: plural, “experiments” [AUTHOR] Section has been rewritten

[REF] P4925 L2-5: Needs a rewrite, maybe splitting into two sentences. [AUTHOR] Section has been rewritten

[REF] P4925 L8-10. Here again I rather question the assertion that WRG has been so relatively ignored as a contributor of discharge. Rignot et al. (2011) clearly highlight the importance of “Sor Rondane Ice Stream” (which is in fact WRG) already. [AUTHOR] It is true that both Rignot et al (2011) and Pattyn et al (2005) identify the glacier as a distinct ice flow feature in the region, but none of them present it as a major contributor.

[REF] P4926 L12: I question the phrase “devoid of distinct lateral constraints.” This goes back to the points I made in my Q3 above. [AUTHOR] See our response above

[REF] Section 8: Is this section actually necessary? [AUTHOR] No, we removed it.

[REF] P4930 L21 sp. Database

[REF] Fig 1 caption. Is the phrase “a couple of PALSAR imagery” correct? [AUTHOR] Replaced couple by pair

[REF] You also need to clarify in the caption that the annotations SRM and BH refer to Sor Rondane Mountains and Belgica Highlands respectively. [AUTHOR] Done

[REF] Fig 2. It would be useful if the 7 cross profiles were annotated 1 to 7 in this diagram to speed up ease of reference when flicking between the text and this diagram. [AUTHOR] Done

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[REF] Fig. 3 This diagram would be really helped if some surface-velocity contours could be added to guide the viewer as to the main flow over the area, so as better to compare with the subglacial roughness signal. [AUTHOR] Done See figure uploaded.

[REF] Fig 3 caption line 3 should also read “result” (not “results”) [AUTHOR] Done

[REF] and line 4 should read “corresponds to a” instead of “refers”. [AUTHOR] Done

[REF] Fig 4c. It's really hard to see three distinct lines in this diagram, and although the statement is made in the caption that colours correspond to those in Fig 5, I think it would also help to see A,B and C labelled. As I've mentioned above I'm also not clear why Fig 4b is even presented. If it were not, the vertical scaling of Figs 4c and d could be improved, better to see the distinct lines (even if one point of presenting Figc is to show general agreement between models). [AUTHOR] See our remarks above. We did remove the ensemble of lines and stuck to one.

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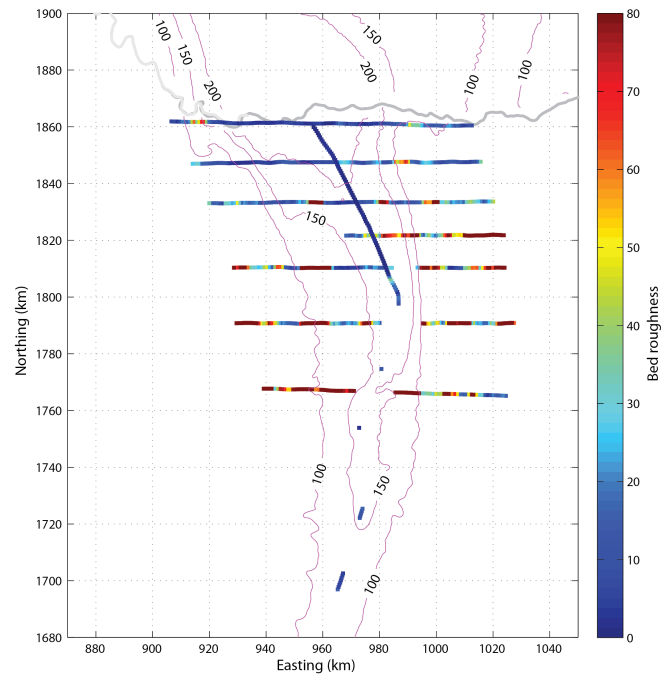


Fig. 1. Fig 3

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